IS OVARIOTOMY JUSTIFIABLE, OR NOT?

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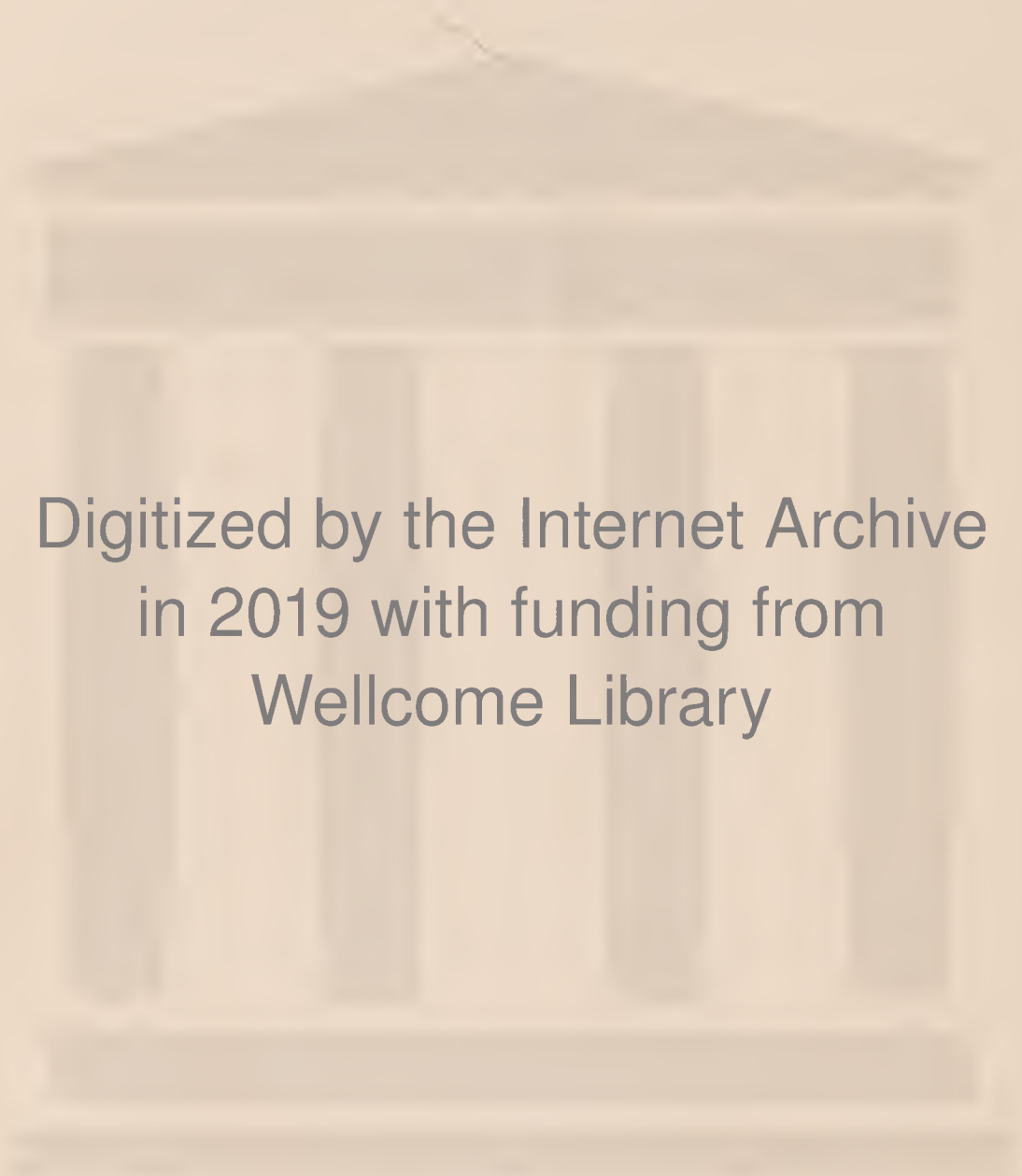

IS OVARIOTOMY JUSTIFIABLE, OR NOT?[1]

THE question, whether ovariotomy is, or is not, an operation that should be resorted to for the cure of any class of cases of ovarian dropsy, has been recently the subject of renewed discussion in the Medico-Chirurgical Society of Edinburgh. My own opinion was there stated,[2] to the effect, that although individual cases might possibly occur where resort to the operation was justifiable, yet that there was no class of cases of the disease for which it was a suitable therapeutic measure. The observations, however, made by myself in that Society require further enforcement and enlargement.

First of all, it is easy to see that the defenders of the operation in that Society have involved themselves in a dilemma. They tell us that the operation is as justifiable as any of the great operations of surgery. They sanction and commend the practice of Dr Clay, as a whole. They admire and hold up the results of his numerous operations. They colour their descriptions of the disease with as much danger as they can make adhere to it. They do the same in regard to the alternative palliative treatment by tapping. These gentlemen, pursuing this line of argument, are in extensive practice. Taken together, they are ever seeing, I believe, as many cases of ovarian dropsy as any equal number of obstetricians that ever met to defend ovariotomy. And yet, incredible to relate, they have only one case of the operation to show for years of experience in the treatment of this disease. More incredible still, the palliative treatment, which they vilify and asperse, is the treatment which, it is notorious, they adopt. The position of my friends, Dr Clay and Mr Edwards, is easily admitted as reasonable. They believe ovariotomy is a good and justifiable treatment in a certain class of cases of this disease; they resort to it, and recommend it to their patients. The defenders of ovariotomy, who strove in that Society to overthrow my reasoning in regard to it, act as I do. They have to explain how it is that their practice is different from their profession. At present, they are in a position which, for character's sake, they must desert; for they defend an operation as a good and salutary measure, as saving life, and yet they do not perform it; nor do they get their surgical friends to do it for them.

It would be difficult to *demonstrate* that ovariotomy is an unjustifiable operation. In the sequel, it will be seen that I refer all such difficult and complicated practical questions as this to the arbitrement of professional opinion, as the

[1] From the *Lancet* of February 28, 1857.

[2] See *Edinburgh Medical Journal* for February 1857, p. 752.

ultimate resort. It is well known that professional opinion is, generally speaking, very decided against the propriety of ovariotomy as a remedy in ovarian dropsy. But on whatever side professional opinion may be found, it is not incumbent on the opponents of ovariotomy to do more than show how all the arguments in defence of the operation are successfully assailed. It is, however, the manifest duty of the defenders of the operation to do all they can to acquire for it the position they desire.

In framing defences in future, ovariotomists must, to use an idiom, make the operation speak for itself. The statistical arguments adduced, in form of comparisons of ovariotomy with other recognised operations, have two great sources of weakness. For, firstly, as we shall immediately point out, the statistical arguments are conducted with such looseness and disregard of logic as to destroy their value. Secondly, if the statistical arguments were well established, it could justly be objected that they prove nothing, unless it be admitted that the objects of comparison were themselves justifiable. If, for example, the statistical comparison between a hundred ovariotomies and a hundred amputations of the thigh were made to yield a result favourable to ovariotomy, it would still have to be shown that the amputations were justifiable. The fact that one operation is as justifiable as another, does nothing towards showing that either one or the other is itself essentially good. All that we can, with our present data, perform, is merely to make an approximation to an argumentative solution of the question of ovariotomy. Before a conclusive proof could be led on either side, it would be necessary to settle many points in surgical ethics which have not yet been mooted in this question, but which some statisticians assume in their own favour. Some of these I shall here merely raise, without saying more than that I am inclined to think they must be answered in the negative.

Can a surgeon or physician, with safety or advantage, bring distant statistical arguments to the bedside of a patient? Is not every case rather a matter of separate study, and to be treated by the clinical physician, or surgeon, apart from difficult questions of the application of statistics to therapeutics, and the results of such statistics?

Can a surgeon or physician ever dare to reason statistically as follows?—I have four cases, all destined to an early death. I shall subject one patient to quick destruction, in order to secure for three the ordinary chances of life.

Can a physician or surgeon ever dare to reason statistically, as follows?—I have four patients, all of whom may live to the natural term, but will probably die within six years. I shall subject one to quick destruction, in order to secure for three the ordinary chances of life. Has any man a right so to deal with human life?

Authors, in general, treat this subject in a curiously inconsequent way. For instance, in the Medico-Chirurgical Society, Dr W. T. Gairdner justly pointed out the two aspects, one of which most cases of ovarian dropsy presented. In the one, the circumstances of the case were consistent with continued life and some degree of comfort, and the operation was too dangerous to be recommended. In the other, the disease was far advanced, the patient's health much injured, and the whole constitution in a state very unfavourable for the operation. But Dr Rigby, a defender of ovariotomy, in his interesting work recently published,

points out, in a similar way, the two aspects of cases of ovarian disease, and yet recommends the operation. Dr Gairdner had never seen a case suitable for ovariotomy, a circumstance quite in accordance with his statement. Dr Rigby approves of the operation, but so encumbers with conditions the two classes of cases of ovarian disease—1. The generally healthy and comfortable, and unsuited for operation; and, 2. The aggravated cases unsuited for operation,—that none are left for the surgeon's knife.

Another instance may be given from the discussion in the Society. Dr Simpson then said, that "he particularly doubted whether surgeons were justified in so often subjecting patients to a great chance of speedy death, from a severe surgical operation for the removal of a disease which might still allow of the continuance of life for many months or years, before it would probably, in the common course of the malady, reach a final and fatal termination."[1] These remarks are, I believe, very just, and the doubt very proper. But then Dr Simpson has no such remarks on ovariotomy, and no doubt about it!—an operation, to which the remarks and the doubt were more appropriate than to any other.

Another illustration is too apposite to be passed over. Dr Simpson supposes that, by means of Dr Southam's table of 20 cases of tapping, he shows first tappings to have a mortality of 1 in 5. He states that he has had about 30 cases of injection of iodine after tapping. He says none of these cases was fatal. In one, indeed, the patient died; but he ascribes this result to the tapping, not to the injection. Arguing from the great mortality of simple tapping, and the asserted absolute innocuity of tapping with the injection of iodine, he is bound to adopt the absurd conclusion, that tapping followed by iodine injection is infinitely safer than tapping alone!

THE LOOSE AND ILLOGICAL USE OF STATISTICS.

Statisticians are justly proud of the value of the numerical method of inquiry, and can point to many proofs of its uses and advantages. But, unfortunately, the opponents of statistics can be at no loss to find ample evidence of its being a method worthy of little confidence, when wielded without sufficient knowledge and care. This has been frequently pointed out by statisticians themselves, and medical philosophers have uttered ominous warnings to their fellow-inquirers not to confide in them in questions of therapeutics, such as the one now under consideration. But in vain.

The statistical argument in favour of ovariotomy has been used by Southam, Safford Lee, and with the greatest ingenuity by Dr Simpson. It was stated, several years ago, at great length in the Medico-Chirurgical Society. It was conducted by comparing the statistics of ovariotomy with the statistics of other operations. Some of the grand errors in that statistical comparison it is necessary to point out.

1. The comparison, if intended to yield results in favour of ovariotomy or against any other operation, must be confined to those operations, and conducted to a termination. Afterwards, the like may be done in regard to some other surgical operation, and conducted to a termination. Instead of this, the statistics

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[1] *Edinburgh Medical Journal* for February, p. 757.

of all surgery are rummaged for arguments in favour of ovariotomy, and a triumph proclaimed in its honour, because all the difficulties and dangers of the most severe operations are not found in connection with it. Is it desired, for instance, to extenuate the danger and mortality of ovariotomy? Then the statistician easily adduces operations with a greater average fatality—amputations of the thigh (and of the arm!)—ligature of the subclavian artery or of the innominata. Is it desired to screen the difficulties of ovariotomy? Then the difficulties of lithotomy, of tying arteries, are adduced, etc., etc.

2. For the purposes of useful comparison, it is necessary that the objects compared have their prominent characteristics in common. Any essential difference must, at least, be pointed out. But, instead of this, we have, for instance, ovariotomy compared to amputation at the hip-joint or of the thigh. Ovariotomy need not be described: it has a distinct individual character. But amputations are of very different kinds or classes, and these, for all useful purposes, totally unknown, and certainly undescribed by the statistician. The comparison might justly be made in regard to mere mechanical circumstances of the amputation, thus: seventy amputations at the hip-joint have been done, and so have seventy ovariotomies! In carrying the comparison farther, the statistician is but a blind leader of the blind. The average of deaths after ovariotomy is less than after amputation at the hip-joint. This proves nothing in any direction. Were the operations, like ovariotomy, for chronic disease? It is not known. Were the amputations performed for accidents, in themselves almost necessarily fatal? It is not known. Were they for malignant disease? It is not known. Were they for gangrene of the limb after fever, or ligature of an artery? It is not known. In short, the whole comparison is done in total darkness.

3. For the purpose of a useful comparison, the circumstances of the operation must be nearly alike. But instead of this, the statisticians place ovariotomies done in the most favourable circumstances, watched with the tenderest care, against operations done in hospital, on young and old, on temperate and intemperate, etc., etc.

4. For the sake of justice, it is necessary to compare the statistical results with the antecedents of the operations. For a greater fatality in amputations than in ovariotomies is quite consistent with the amputation being, in spite of that circumstance, the more justifiable, and even, in a sense, the safer operation, For the amputations may (and very probably) have been all done in cases quickly, and certainly tending to a fatal termination; and a small fraction saved may prove a far greater triumph of surgical skill, than a larger number or fraction saved after ovariotomy. In the ovariotomies, death was possibly far from being near at hand in many, if not most of the cases. Some of the dead might have long survived but for the surgeon's knife.

If statistics are to be used in such a loose fashion as I have described, it may be truly said, that by their help, no absurdity need despair of evidence. But I proceed to another aspect of this method of advancing medical science.

THE ABSURD USE OF STATISTICS.

When, in the Medico-Chirurgical Society, I pointed out the statistical conclusion, that tapping was fatal to one in every five operated on, as a glaring instance

of the absurdities into which statistics allured those who failed to use them aright, I was told that the great mortality in the operation attached itself to first tappings; and this formed the whole justification of Southam's well known table. Of this table of twenty cases, Dr Simpson says—"Fifteen of these cases had been recorded by Drs Bright and Barlow, without, apparently, any view to such an investigation, and hence afforded the more valuable and unprejudiced evidence. Four of the twenty, or one in five, died of the effects of the first tapping."[1] It is a curious, but vain endeavour, to conceive how Drs Bright and Barlow could illustrate the danger of tapping in a valuable and unprejudiced manner, because they had no intention of illustrating it at all. The exposing of the real circumstances of this table, and of the arguments founded on it, will form to future inquirers a valuable warning against putting faith in statistics, when used to support any practice whose promoters are struggling for defence.

The table, then, is used by Safford Lee, Simpson, and others, to show that the first tapping in ovarian dropsy is a proceeding nearly as dangerous as ovariotomy—that the mortality from it is about one in five.

If Drs Bright and Barlow had published all their hospital cases of tapping, or all their private cases, then we might have had data of some value. But what is the fact? Dr Bright's paper, from which the table is got up,[2] contains the histories of twenty-four selected cases of ovarian disease, all of which (with two exceptions) are completed by accounts of the *post-mortem* examinations. Most of them were women coming into hospital with the disease in an advanced stage. These cases were selected by Dr Bright, and wisely so, to illustrate the terminations of the disease. Some of them were cases of malignant disease. It is almost too ridiculous to be believed, that these cases should be used in reference to the question of first or subsequent tappings.

Of the four so-called fatal cases of first tapping in Southam's table, three are drawn from Dr Bright's able paper, in the Guy's Hospital Reports. Let us examine them briefly. 1. In Dr Bright's words, "she could walk from Peckham to London and back, and she was fond of dancing.—June 18, 1831.—She was tapped in the middle line, about an inch below the umbilicus: a few drams only of fluid came away; when a little cyst protruded, almost like an hydatid; but it was attached within, and was returned; a small quantity of blood escaped. Within an hour or two of the operation she began to experience collapse, and died within twenty-four hours." This is evidently an example of death from tapping. Dr Bright does not say it was a first tapping. It is not unimportant to observe, that it is quite an exceptional case, on account of the circumstances of the hydatid and the escape of blood, etc., etc. Moreover, it is very doubtful if palliative tapping includes an operation on a woman who was a strong walker, and fond of dancing. The title of the case makes it evident that it is related because it was fatal after the tapping.

2. This case is also selected, in order to illustrate death from tapping. Dr Bright does not say whether the fatal tapping was a first operation or not. The statisticians assume it.

3. This case was, according to Dr Bright's account, not one of a first tapping;

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[1] *Obstetric Works*, vol. i., p. 266.

[2] *Guy's Hospital Reports*, vol. iii.

for he says, "The fluid in the cyst differed entirely from that which had been drawn off two months before." The case was not under Dr Bright's immediate care, and death was the result of the first of an intended series of tappings to be tried, after a peculiar method, as an experiment.

4. To make up the four fatal cases, one is taken from Dr Barlow's paper. In this case, it is not stated whether the tapping was a first operation or not. Mr Abernethy, writing of this case, said:—"I do not remember a diseased ovary advancing with such continued irritability or disposition to inflammatory action." Dr Barlow's description is as follows:—"Enlargement proceeded rapidly; but fluctuation became indistinct, and at length ceased to be felt. Much suffering was endured, which terminated in death towards the end of October. A short period before death, an attempt was made to relieve the oppressive distension by tapping, but unsuccessfully." The perusal of this case leaves the reader without the slightest ground for thinking the tapping was the cause of death—quite the reverse. Dr Barlow's whole paper consists, like Dr Bright's, of cases so selected as to illustrate points in the pathology of this interesting disease.[1]

But the climax of absurdity is reached in this argument; for I find that Dr Southam's table of twenty cases is not one of first tappings. Of the twenty, eleven had been repeatedly tapped. Nine only are *said to be* cases of first tapping. They were all followed by death; and it will puzzle the wittiest to explain why the four cases above described were selected from the whole twenty, to strike an average of one death in every five first tappings. If the table proves anything (which I doubt), it proves that every first tapping is fatal! and that, after tapping, a woman still must die some time or other!

I need say no more, for enough has appeared to show that the bases, superstructure, and uses of these statistics, are not only worthless, but ridiculous. It is not my purpose, at present, to discuss the mortality of tapping. No doubt, it has a mortality—so has phlebotomy, says M. Velpeau.

In the discussion so often alluded to, more than one speaker disparaged what was called, very appropriately, "surgical instinct." This phrase was used to indicate the opinions of great and wise practical men, arrived at, none the less surely, because, to some extent, by a series of logical steps, which they cared neither to investigate nor discover. The disparagement was thrown on their own profession, and on themselves. It was a self-destructive act. None of the speakers made a good defence of ovariotomy, and if they had fallen back on their opinions, would have been, in some sense, impregnable. The opinions of great and wise practical men are, and will be, the great resting-place of the profession, and of the public. These men are almost all inimical to the operation under discussion. Many of them flatly repudiate it a place in regular surgery. Others, like Professor Miller, arrive at the same result, by encumbering it with impossible conditions.

Casting contempt on surgical instincts, what have the defenders of ovariotomy to offer us instead? Nothing but flimsy and fallacious arguments of the kind considered above.

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[1] *Trans. of the Provincial Med. and Surg. Association*, vol. iv.

REMARKS MADE IN THE MEDICO-CHIRURGICAL SOCIETY, AFTER THE READING OF MR EDWARDS' CASE OF OVARIOTOMY.

Dr Matthews Duncan, in rising to open a discussion upon Mr Edwards' paper, desired, first of all, to say, that he should abstain from commenting on the special case brought forward, in which, he doubted not, the operation of ovariotomy had been performed with skill, and the after-treatment conducted with attention and assiduity. He would proceed to make remarks on the great question which this case of operation raised, viz., whether or not ovariotomy was to be recognised in surgery as a justifiable operation for the relief of ovarian dropsy in general, or of any class of cases of that disease. His own opinion was, that, although ovariotomy might possibly be justifiable in certain individual cases of ovarian disease, yet that it did not deserve to be recognised as a remedial measure in any class of cases of the affection. The question whether ovariotomy were justifiable or not, he considered one of the most important that could be discussed in such a meeting as this. Patients labouring under this dangerous disease, were far from rare; and some practitioners might often feel difficulty in giving advice on the subject. The boldest and most rash practice was often preferred by patients to what might appear to be more cautious and less heroic. Recommending the latter, as he did, he had no desire to be considered deficient in boldness, just as the ovariotomists would deprecate their being condemned as wanting in caution and prudence. He, therefore, wished to state his impressions in regard to the arguments adduced in justification of this excessively dangerous proceeding.

In defending, some years ago, before this Society, his unsuccessful case of ovariotomy, Dr Handyside said, that the only justification and warrant for operations involving imminent peril and hazard, was acknowledged to rest on this ground, that *their performance was essential to the preservation of the patient's life.* In this defence, Dr Handyside had laid down an axiom which was, Dr D. feared, too strict. A great number of the most common and useful operations in surgery would be unjustifiable on any such general principle. In ovarian disease, no one could tell how long life might be prolonged in almost every case that came under observation. The chances in favour of a woman were very considerable, at least in every case that a surgeon would choose to subject to the operation.

Disparaging reference had been made by Mr Edwards, to what was, with much appropriateness, called the instinct of surgeons. These so called instincts were merely the opinions of practical men, arrived at, none the less surely, because, to some extent, by a series of steps in reasoning which they did not care to discover or investigate. Great practical men, in all professions, were very often weak in dialectics. A great admiral or a great surgeon was the best man to trust to on a practical question, although such admiral or surgeon did not seek, or, if they sought, failed to convince all men of the correctness of their own views. In a complicated question, such as that of the propriety of ovariotomy, the last and most satisfactory resource was the deliberate opinion of great and wise surgeons, and Dr D. was sure that the world and the profes-

sion, true to itself, would look on these opinions in the same light as he did. It was well known that surgeons, like men of other crafts, were anxious to claim as great a field for their operations as was at all justifiable—a circumstance which lent weight to their decision, when it was against the use of those instruments of which they were justly proud. Now, what was the fact? It was that the most able and wise surgeons of this country, and of the continent, repudiated this operation, and refused to give it that place in surgery which some wished to claim for it. It was, no doubt, lucky for the ovariotomists that surgeons had failed to put in writing a logical proof against the propriety of this kind of interference, which, however, many of them were, no doubt, capable of framing. It was surely impossible for any surgeon here to be an approver of the use of this operation, as it had been elsewhere practised, else we should have had now a large number of cases before us, instead of a single one.

The arguments most frequently referred to, in justification of ovariotomy, were based on statistics. They were, for the most part, adduced by Professor Simpson, in this Society, several years ago. Dr Duncan was as anxious as any one could be, to increase the well-deserved confidence of the profession in statistics, when properly used. To secure this last condition, the strictest attention to the cold rules of logic was required. Without this, the dangers of false reasoning were always very great, and especially so in therapeutical questions. In these last, he feared that statistics had hitherto done more harm than good. He quite agreed with a distinguished surgeon, who, on the last occasion on which statistics were discussed in this Society, said that, if any one wished to prove black to be white, he should resort to statistical arguments. No better example could be got of the extremely absurd results which statistics might be tortured to support, than was afforded in the question before them. Ovariotomy was shown, by the statistics adduced, to be fatal to the patient in one out of every three or four cases. For the justification of this operation, it was considered necessary to make out tapping in the same disease to appear as dangerous as statistics could make it; and Dr Simpson adduced some data which he thought valuable for this purpose, and which showed that tapping was fatal in about one in every five cases operated on. The operation of tapping was one of daily performance, and every body knew it was attended with little danger. Many women had had it performed fifty or a hundred times, without the least bad symptom. Dr D. had never seen any evil from it—he had performed it on a woman the forty-sixth time. No doubt it was an operation occasionally followed by lamentable results, with which it had generally no connection. It was sometimes done on patients as a last and almost hopeless remedy; and the evil result in these cases was no more to be attributed to it than to the last dose the patient might have swallowed. Yet, wonderful to relate, statistics had shown tapping to be nearly as dangerous as ripping up a woman from pubis to sternum, and taking out a large abdominal tumour! If the justifiers of ovariotomy began thus, what could be expected of them afterwards?

The great statistical argument was, that many other recognised surgical operations had as large a mortality as ovariotomy, and that, therefore, ovariotomy was as justifiable as they were. But, after a little reflection, it would be evident to any one that this did not follow. Dr D., however, denied

the first assumption in this argument, namely, the justice of the comparison made between ovariotomy and other operations. For justice' sake, it was necessary to compare ovariotomy—an operation having a distinct individuality—with some other operation having the like. But, instead of this, a rambling comparison was made with a great variety of operations. To take the single operation chiefly employed in these questions, namely, amputation of the thigh, we found ovariotomy, an operation performed in the favourable atmosphere of a private house, on a selected patient, and for a disease quite consistent with a moderate, and occasionally with a long continuance of life—we found, he said, ovariotomy compared with amputation of the thigh performed in an hospital, mostly for affections which afforded no escape with life, except after the operation—affections, also, of the most varied description, the patients being often in the most unfavourable circumstances, suffering from the most horrid and complicated accidents, from intemperance, and a number of other unfavourable conditions. How any just comparison could be arrived at in this way, it was impossible to see. It would be necessary, to secure anything near the demands of the question, to take a series of amputations of the thigh for chronic disease, performed in circumstances equally favourable with those in which ovariotomy was performed. Dr D. did not doubt that, were this done, the result would be to show ovariotomy far to exceed the mortality of almost all recognised surgical operations, for even the most hopeless diseases. An old surgeon in the Royal Infirmary had had but one death among fifteen consecutive cases of amputation of the thigh, as these turned up in the practice of the institution. If an ovariotomist could produce anything like this, Dr D. would modify his views of ovariotomy. The fact, however, was, that no statistics existed fit for such comparisons as had been instituted. The farther we cautiously advanced in this difficult question, the more were we forced to resort to the opinions of great and wise surgeons—and they condemned the operation. The uniformly unfavourable results of the operation in this neighbourhood were surely enough to deter most men from attempting it. Dr Duncan concluded with some remarks on the new treatment of ovarian dropsy by iodine injection. The introduction of this great remedy formed an era in the treatment of all serous cysts and numerous open morbid cavities. In the treatment of ovarian dropsy, it was still to be considered as *sub judice*. Recently it had been brought much into notice by M. Boinet, who had written a work on this treatment which he called *Iodotherapie*. The experience of French surgeons, now very extensive, was not very favourable to this treatment; for, while it appeared to be a very valuable, and comparatively safe remedy in ovarian cysts, and cysts of the ovarian region, which were unilocular, and had a thin serous fluid for contents, it was otherwise in the commoner disease called ovarian dropsy in this country. It was in ovarian dropsy that it was, in this country, recommended—that is, in the common multilocular cyst, with viscid albuminous contents. Now, the best observers had found that, in such cases, it occasionally retarded or arrested the refilling of the principal cyst—that is, the cyst usually tapped; but it was also frequently followed by no good result, while it was attended by a risk to life far greater than in the various kinds of unilocular cysts, in which it appeared to be really a very effectual remedy.